This in itself disposes of his theory that the germinal matter in the non-encased tubes is destroyed by solar *heat;* for if that heat were sufficient for such a result, it should obviously suffice also for the destruction of germs contained in the encased cultivation liquid.

Professor Tyndall, in repeating our experiments, is forced to the same conclusion, namely—that the energy which here prevents putrefaction is energy in the radiant form.

Secondly, Dr. Jamieson will find in the second of the papers in the *Proceedings of the Royal Society* details of experiments which distinctly show that the waves of greatest refrangibility are the most active; in other words, to use the old phraseology, that the effect is associated chiefly with the "actinic" rays. This fact, which may readily be substantiated by any one who will carefully repeat our experiments, must again prove that Dr. Jamieson's supposition of heat destruction is quite untenable.

Art. II.—*The Influence of Light on Bacteria.*

By James Jamieson, M.D.

[Read 12th April, 1883.]

At the meeting of this Society on 8th June last I read a paper on this subject, in which I detailed the results of certain experiments, made for the purpose of testing the conclusions arrived at by Professor Tyndall, and by Messrs. Downes and Blunt. I was led at first to agree fully with these gentlemen, that the effect of exposure to the sun's rays of solutions inoculated with bacterial germs is to prevent the development of the bacteria. Continued observation, however, showed me that the fullest exposure to diffused light has no such effect; and, further, that long continued exposure to the direct rays of the sun need not have that effect. Finding, also, that insolation seemed to fail when the temperature was moderate in degree, I was led, perhaps rashly, to conclude that the destructive influence was exerted, not by direct sunlight *per se*, but by the elevated temperature accompanying it. This conclusion seemed all the more reasonable, since degrees of temperature were actually attained, which are known, if continued long enough, to be destructive to the *Bacterum termo*, the organism under investigation. Whether my interpretation of the nature of the injurious influence at work was a correct one or not, it was certainly shown by my later experiments, (Exps. VI. and VII., *Transactions Roy. Soc. Vict.* 1882, p. 120), that exposure to the sun's rays, for several days continuously, need not destroy, or even apparently retard the development of, bacteria in a perfectly transparent nutritive solution. As a matter of fact, development in one case (Exp. VI.) went on most rapidly in the one of three bottles, which had been exposed continuously for the longest time. If variation of temperature was not the determining cause of the different reaction shown by these three samples of bacterialised solution, then I know not how to explain that difference.

Dr. Downes, however, not being satisfied with my criticism of the conclusions arrived at by himself and Mr. Blunt, has forwarded to this Society the short communication just read. With reference to that communication, I must first say that the suggestion offered that I could not have seen the text of the papers in the *Proceedings of the Royal Society* is not correct; and the exactness of my references and quotations ought to have shown that I had read them. With the arguments used to show that my conclusions were not well founded, and that theirs were not open to criticism, I need not take up much time. I have found, in agreement with Dr. Downes, that an inoculated solution, exposed to light coming through red glass, becomes turbid sooner than a similar solution cultivated under yellow glass, and that it may remain long transparent under exposure to light reaching it through blue glass; but it does not seem to me of necessity to follow, that the mixed rays in white light, even of great intensity, must be destructive. I have also tested the comparative temperature of solutions, in bottles cased in tinfoil and naked, and have not found it uniformly higher in the former, when both are exposed to the sun. I can easily understand, in fact, that bottles or test-tubes, wrapped all over in foil or any other covering, and standing on a hot surface, such as a windowsill on which the sun's rays strike, may be better protected by the wrapping from the heat of the surface on which they rest than others not so wrapped. The temperature attained under these circumstances will depend, in fact, more on the height of the column of fluid than on the mere difference of wrapping or no wrapping. The high *à priori* method which Dr. Downes adopts in his communication is, I venture to think, not quite appropriate in an inquiry, in which direct experiment is applicable, and can, indeed, alone be conclusive. An illustration of the danger in applying this method may be taken from the first paper of Messrs. Downes and Blunt (*Proc. Roy. Soc.*, 1877, pp. 499, 500). They found that, of tubes containing urine exhausted with a Sprengel pump, those which were insolated became turbid sooner than those which were encased. This experiment may not have proved that insolation favours the development of bacteria, but it surely may be taken as showing that insolation *per se* is not excessively destructive.

I may have been wrong in attributing too much influence to an elevated temperature *per se*; but I must still insist that Messrs. Downes and Blunt gave too little consideration to it as at least a disturbing element, recognising it only as a condition favourable to development.

In my previous paper I did not venture to deny to direct sunlight any influence whatever inimical to the development of bacteria, though I did not think that that inimical influence was established by the experiments described. I have felt it incumbent on me to repeat, with variations, the investigations previously reported, and though perhaps even less disposed than I was then to consider light a mere neutral factor, I am still compelled to repeat that bright light, and even direct insolation, need not prevent the development of bacteria in nutritive solutions. A short account of one or two experiments, out of a considerable series, will suffice to show both methods and results:—

Exp. I. Five one-ounce phials were charged equally with about a dram and a half of inoculated Cohn's solution, and plugged with cotton wadding. Three were suspended outside of a window, receiving the direct rays of the sun for the greater part of the day. Of the three, one was wrapped in brown paper, the others left uncovered. One bottle was left standing outside uncovered on the stone windowsill, and one was placed for comparison on a shelf in a tolerably well-lighted room, the sun's rays falling on it for an hour or so in the afternoon. This was on 12th February, the day being very hot. The 13th was cool and cloudy, the 14th bright and warm; and on the 15th, which was also bright and very hot, the solution in the bottle kept inside was already opalescent in the morning, the wrapped suspended one likewise opalescent later in the day, both rapidly becoming quite milky. The other three were still transparent. On 2nd March both of the exposed suspended bottles began to show a slight milkiness, which by the 8th had increased to complete opacity. Even at this last date the one left standing on the windowsill uncovered was still quite transparent. The general results of this mixed experiment were —first, that a solution exposed to diffused light, and even to some extent to the direct rays of the sun, developed bacteria as quickly as that contained in a bottle carefully wrapped in paper; and, secondly, that bottles suspended in the sun showed full development of bacteria, though at a later date, while one which had been standing on a hot window sill continued to be quite transparent. The amount of light was not greater in the latter case, but the temperature attained in the sun was considerably higher; and I cannot think of anything but this difference of temperature which could have brought about the different results. The actual difference in the temperature of the solutions, in bottles standing and suspended, is very considerable, since I found that, with the thermometer at about 118 degs. Fahr. in the sun, fluid in the bottom of a bottle, standing on a windowsill beside it, rose readily to 108 degs. Fahr.; while fluid in suspended bottles, whether naked or covered with tinfoil, rose only to 98-102 degs. Fahr., when the thermometer marked as much as 125-132 degs. Fahr.

The difficulty I have experienced in carrying out comparative tests lay in preserving uniformity of temperature, with varying intensity of solar light. I tried first to get over the difficulty in the following way:—

Exp. II.—Two bottles, each containing two drams of inoculated solution, were suspended inside but just behind the glass of a high window, on which the sun fell nearly all day. One was wrapped in paper, the other exposed. This was on 19th February at two p.m., the day being bright but cool. The 20th was cloudy in the afternoon, the 21st bright and warm, and on the 22nd the solution in both was distinctly opalescent, though most markedly so in the covered one. On the 24th both were quite milky, but still the bacterial growth was most marked in the wrapped bottle.

The doubt was whether the more rapid development in the covered bottle was due to the protection from the light, or to the more uniform temperature preserved by the paper wrapping. I therefore varied the conditions in the following way :—

Exp. III.—Three small thin phials were half filled with inoculated solution, and suspended just inside of a window, as in the last experiment, on 6th March at noon, the day being bright and warm. One of them was not protected at all from the sun ; the second was shielded from its rays by a small piece of thin white paper put between it and the glass of the window ; while the third was more fully protected by means of a larger piece of thick brown paper. The 7th was bright and very hot ; the 8th warm, but cloudy after the morning. On the 9th, at 9 a.m., both the protected bottles showed slight opalescence, which steadily increased, though without noticeable difference in them. Only on the 11th was there slight cloudiness in the exposed bottle, which became distinct on the 14th ; and on the 19th, after several very clear, hot days, it was quite milky and crusted. It may seem that the influence of the direct rays of the sun in retarding development is here quite apparent. That the retardation may in part have been owing to that I am not prepared absolutely to deny ; but it is also evident that the unprotected bottle was also exposed during the day to a higher temperature than the others, and possibly also to a slightly lower temperature during the night, and thus to greater fluctuations, both upwards and downwards toward unfavourable extremes. I have not been able to devise any arrangement whereby a nearer approach than in this case could be got to uniformity of temperature with varying intensities of light. I claim, however, to have again shown clearly, in opposition to the conclusions of Messrs. Downer and Blunt—

(1) That the brightest diffused light is not inimical to the development of bacteria ; and (2) that full exposure to the sun's rays is not destructive to bacteria or their germs, when precautions are taken, as by suspension, against exposure to too high degrees of temperature.

I cannot add that such exposure to the sun's rays in no way retards development, but I must express the conviction that retardation may generally with equal propriety be ascribed to extremes of temperature associated with the insolation.